Appendix A

The Impossibility of Calculating Service-Specific TFP for Interstate Access

It is well known in the theory of production literature that when outputs use factors of production in common, separate production functions cannot be defined (see Hall (1973), Denny and Pinto (1978, Chambers (1988)). Since interstate services use factors of production in common with other services, a separate production function cannot be defined for interstate services.

To be more specific, suppose the telecommunications firm produces two outputs: interstate (Y_1) and intrastate (Y_2), using two inputs X_1 and X_2. Then the production process for the multiproduct firm can be written as

$$F[Y_1, Y_2, X_1, X_2] = 0 \qquad (1)$$

In the special case where the production function is separable, (1) can be written in the form

$$F[Y_1(X_1, X_2), Y_2(X_1, X_2)] = 0 \qquad (2)$$

where $Y_1(X_1,X_2)$ and $Y_2(X_1,X_2)$ are separate production functions for the production of Y_1 and Y_2 respectively.[16]

[16] From a technical perspective, (2) implies that the marginal rate of substitution between the two inputs in the production of Y_1 does not depend on the level of Y_2, and vice versa.

Equations (1) and (2) can also be written in terms of cost functions. Suppose p_1 and p_2 are the factor prices for the two inputs. Then the cost function for the general production function (1) can be written as

$$C = C(p_1,p_2,Y_1,Y_2) \tag{3}$$

where C is the firm's total cost of producing the two outputs. In the special case where the production function is separable (equation (2)), the cost function can be written in the form

$$C = C_1(p_1,p_2,Y_1) + C_2(p_1,p_2,Y_2) \tag{4}$$

where C_1 and C_2 are the costs of producing output 1 and output 2 respectively. Only in the case where the cost function can be written in the form (4) can total costs be **separated** in any economically meaningful way into the costs of producing output 1 and the costs of producing output 2. And only in this case is it economically meaningful to measure separate TFPs for the two services.

The cost function can only be written in the form (4) if there are no economies of scope in the production of the two outputs. Economies of scope result when inputs are used in common by the two services. Clearly this is the case in telecommunications, where a substantial amount of labor and especially capital are jointly used by interstate and intrastate services. As noted earlier, there is no economically meaningful way to separate the costs of such jointly used inputs in order to calculate service-specific TFP growth.[17]

[17] The only efficiency-related concept which can be calculated at the level of a specific service is the decline in marginal cost which may occur when outputs grow. Growth in interstate output can lead to a decline in the marginal cost of producing interstate services (and hence a decline in the interstate price required by the firm) through economies of scale. But this growth can also lead to a decline in the marginal cost of intrastate services (and hence a decline in the intrastate price required by the firm) through economies of scope. Similarly, growth in intrastate output can lead to declines in the marginal costs of both interstate and intrastate services. The relative impacts of economies of scale and economies of scope on the prices required by the firm over time cannot be determined unless a detailed knowledge of the joint cost function (equation (3) in Appendix A) is available. Even if such knowledge were available, service-specific TFPs still could not be calculated since they do not exist conceptually. A single firm-wide TFP estimate would still be the only economically meaningful productivity offset in a price-caps formula.

Appendix B

The Stringency of Norsworthy's Chi-squared Test of the Input Price Differential

Consider the proposed chi-squared test of the equality of the LEC and US input price changes (page 8 of Norsworthy's Statement). Instead of testing whether the two series differ on average over a specific period of time by a random variable with mean zero, Norsworthy proposes a test which tests whether the two series are identical **in each year**. To see the peculiar implications of this test, suppose all parties could agree that the relevant time period for measuring the average input price differential for inclusion in a price caps formula was the period 1949-92. From Norsworthy's table 1, the average increase in the LEC's input prices over this period was 4.70%, and the average increase in the US economy's input prices was 4.75%. It is hard to imagine that a differential of 0.05% would spark much of a debate as to whether or not an average input price differential should be included in the price caps formula. Yet the probability that the two series are the same is an infinitesimal 0.0000000001! Clearly, the chi-squared test proposed by Norsworthy is not relevant to the question of whether a fixed average input price differential should be included in the price caps formula.

Appendix C

Revenue Weights vs Marginal Cost Weights in Price Caps Formulas

The output price index growth rate of a firm, as a matter of algebra, takes the form

$$\text{Output Price Index} = \text{Input Price Index} - TFP^{R} \tag{5}$$

where TFP^R is the revenue-weighted measure of TFP growth (i.e., outputs are aggregated using revenue weights). Denny, Fuss and Waverman (1981) demonstrated that TFP^R could be expressed as

$$TFP^{R} = TFP^{C} + (Y^{R} - Y^{C}) \tag{6}$$

where TFP^C is the marginal cost-weighted measure of TFP growth, Y^R is the growth rate of aggregate output when revenue weights are used in the aggregation formula, and Y^C is the growth rate of aggregate output when marginal cost weights are used in the aggregation formula.

Equation (6) implies that an alternative expression for the output price index growth rate is

$$\begin{aligned}\text{Output Price Index} &= \text{Input Price Index} \\ &\quad - TFP^{C} - (Y^{R} - Y^{C})\end{aligned} \tag{7}$$

As I have recently emphasized (Fuss (1994)), the revenue-weighted measure of TFP growth (TFP^R) only represents efficiency growth if output prices are proportional to marginal costs or the rates of change of all outputs are equal. If at least one of these conditions is not met, efficiency change is more accurately represented by a marginal cost-weighted measure of TFP growth (TFP^C).

While it is clear that when revenue-weighted and cost-weighted TFP growth rates differ historically, the cost-weighted measure is a superior indicator of past efficiency growth, it is not as clear which measure should be used in determining the productivity offset in a price-caps formula. This is because the productivity offset in a price caps formula measures more than efficiency changes; it measures the ability of the firm to sustain output price declines, net of inflation. So, for example, if intensified competition causes a decline in the price-marginal cost margin of a service with a positive margin and the output of that service does not increase sufficiently to offset the margin loss, there will be a reduced ability on the part of the firm to sustain a price index decline, even when efficiency growth is unchanged. This is the reason why, when the output price index is expressed in terms of the cost-weighted TFP measure, an additional term, (Y^R - Y^C), must be included in the equation. This additional term would also need to be included in the price caps formula.

The correct conceptual choice (equation (5) or equation (7)) depends on a comparison of the price/marginal cost relationship in the historical period, from which the productivity offset is drawn, with the relationship expected to prevail in the price caps period. An example drawn from the case of two Canadian telephone companies, Bell Canada and British Columbia Telephone, may clarify the issue. During the 1980s, rates for toll calls exceeded marginal costs and rates for local calls were less than marginal costs. Fuss (1994) demonstrated that this condition, along with the more rapid growth of toll, caused the revenue-weighted TFP growth measure to overestimate substantially efficiency growth. However the revenue-weighted measure might still be the appropriate TFP offset for a price caps plan for these companies. This would occur if the pattern of price, marginal cost relationships were to be continued in the price caps period and there were no significant expected changes in relative growth rates of outputs.

On the other hand, suppose the price caps period represented a period of transition to marginal cost-based pricing; or the price, marginal cost relationships were maintained, but relative output growth rates in the price caps period were expected to differ substantially from the historical period. In that case the conceptually correct productivity offset would be a variable offset which combined the cost-weighted TFP measure with an adjustment term that took into account the changing revenue, cost-weight differentials and the changing relative output growth rates (equation (7)).

While the use of equation (7) would be conceptually correct in the situation described in the last paragraph, it would have several disadvantages from a policy perspective that need to be taken into account. First, marginal cost weights would have to be calculated, and the calculation would likely be controversial. As Norsworthy and Jang (1992) note in the context of US telecommunications, "The use of internal accounting weights to add up the various telecommunication services to a measure of total output is also unlikely to be correct. Accounting practices involve arbitrary methods for allocating fixed costs and major components of variable costs to the various service classes and are unlikely to come reasonably close to the marginal cost weights ..." (page 228).

Second, a price caps formula based on equation (7) would depend on the growth rates of outputs, which creates incentive problems. A LEC would be aware that a lower rate of growth of output for a service which provides a positive margin, or a higher rate of growth for a negative margin service, would result in a lower productivity offset.

A move to a cost-weighted TFP offset would probably be to the advantage of the LECs, in that it would likely result in a lower productivity offset as competition intensifies. This would occur because competitors would target the LECs' high margin services. This targeting would result in reductions in the LECs' price-marginal cost margins and a reduction in the output growth rates for these high margin services. Both impacts would mean that the term (Y^R - Y^C) in equation (7) would decline (or possibly become negative) and the resulting offset would be lower than if the revenue-weighted index were used.

Appendix D

A Comparison of Fisher and Tornquist Indices

The most appropriate way to compare indexing procedures is to utilize the economic theory of index numbers. As developed primarily by Erwin Diewert, the economic theory of index numbers demonstrates that both the Tornquist Index and the Fisher Index belong to the class of superlative indices. A superlative index is an index which corresponds exactly to some second order approximation of an unknown aggregator function which is actually combining the components into an aggregate. From an economic perspective, the Tornquist and Fisher indices only differ because they are exact for different second order approximations. The Tornquist Index is exact for a second order approximating function which is quadratic in the **logarithms** of the components. The Fisher Index is exact for a second order approximating function which is quadratic in the **levels** of the components. Since it is generally unknown which second order approximating function is the better approximation to the true aggregator function, a clear choice is usually not possible. However, for the type of aggregation which is occurring in the Christensen model, it is unlikely to matter. As noted by Diewert (1987, p. 773), in discussing the choice among superlative indexes, "Fortunately, it does not matter very much which of these formulae we choose to use in applications: they will all give the same answer to a reasonably high degree of approximation". The veracity of this quote is demonstrated by the data in table 1 of the text and table D.1 of this appendix.

Since all indexing procedures should be viewed as approximations, there is no conceptual advantage to the fact that the Fisher Price Index is the same approximation numerically whether calculated explicitly or implicitly. The Tornquist indexing procedure gives two distinct approximations to the unknown aggregator function. Either one is as valid as the Fisher approximation. As noted by Diewert, as a practical matter, it is unlikely that the explicit and implicit Tornquist approximations will be significantly different for the data used in the Christensen model. Table D.2 contains a comparison of the two ways of calculating the Tornquist Input Price Index for the Norsworthy data. To three decimal places, the numbers are identical except for 1990, where the numbers differ by one in the third decimal point due to rounding.

Table D.1

A Comparison of Fisher and Tornquist Indices

Norsworthy Data 1985-94

Year	Aggregate Output Index		Aggregate Input Quantity Index		Aggregate Input Price Index	
	Fisher Index	Tornquist Index	Fisher Index	Tornquist Index	Fisher Index	Tornquist Index
1985	1.000	1.000	1.000	1.000	1.000	1.000
1986	1.033	1.033	1.004	1.004	1.055	1.055
1987	1.071	1.071	1.026	1.026	1.052	1.052
1988	1.139	1.139	1.120	1.121	0.985	0.985
1989	1.210	1.210	1.123	1.123	0.992*	0.992
1990	1.289	1.289	1.182	1.182	0.952	0.952
1991	1.350	1.350	1.186	1.187	0.962	0.961
1992	1.402	1.402	1.190	1.190	0.979	0.979
1993	1.470	1.470	1.160	1.160	1.037	1.037
1994	1.555	1.555	1.186	1.186	1.043	1.042

* The corresponding number in Norsworthy's table 5 is 0.982. However, this is a typographical error. The number which appears in Norsworthy's spreadsheet is 0.9925.

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Bell Atlantic

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Joseph J. Mulieri
Director – FCC Relations

June 4, 1996

Ex Parte

William F. Caton
Federal Communications Commission
19191 M Street, N.W. Rm. 222
Washington, D.C. 20554

Re: LEC Price Cap Regulation, CC Docket 94-1

Dear Mr. Caton:

Please find attached a Declaration by Melvin A. Fuss, prepared on behalf of Bell Atlantic, in response to new arguments raised by AT&T and Ad Hoc Telecommunications Users Committee in their reply comments in the above captioned proceeding. Specifically, this paper demonstrates that the claims of the existence of an input price differential, and the possibility of calculating total factor productivity growth for interstate services alone, are flawed. In addition, this declaration further supports Dr. Fuss' original declarations in the this proceeding.

An original and two copies of this ex parte notice and attachment and diskettes are being filed today. Please include this letter, the attached declaration and the diskette into the record as appropriate.

Sincerely,

Joe Mulieri

Attachment

cc w/o disk: J. Farrell, G. Rosston, A. Bush, S. Spaeth, R. Metzger, L. Selzer, L. Huthoefer, J. Jackson

cc: w/disk: ITS

CERTIFICATE OF SERVICE

I, Maureen Keenan, do hereby certify that on this 4th day of June, 1996, a copy of the foregoing Ex-Parte together with the attached declaration and diskette, were mailed by U.S. first class mail, postage prepaid, to the parties listed below:

James Blaszak
Levine, Blaszak, Block & Boothby
1300 Connecticut Avenue, Suite 500
Washington, D.C. 20036

Peter Jacoby
Attorney for AT&T Corp.
AT&T
295 North Maple Avenue
Basking Ridge, New Jersey 07920

Before the
FEDERAL COMMUNICATIONS COMMISSION
Washington, D.C. 20554

In the matter of	)	
	)	
Price Caps Performance Review	)	**CC Docket 94-1**
for Local Exchange Carriers	)	

DECLARATION OF MELVYN A. FUSS

I, Melvyn A. Fuss, declare the following:

Introduction

1. In this declaration, I respond to new arguments raised by AT&T and the Ad Hoc Telecommunications Users Committee in their reply comments regarding their claims of the existence of an input price differential, and the possibility of calculating total factor productivity growth for interstate services alone. I demonstrate that these new arguments are flawed. As a result, the Commission may continue to rely on the conclusions in my original declarations that there is no permanent input price differential and that there can be no economically meaningful calculation of interstate-only TFP growth. In this declaration, I specifically demonstrate the following:

(1) Drs Norsworthy and Berndt's attacks (on behalf of AT&T) on my evidence that there is no permanent input price differential is, in reality, an attack on the methodology of Bush and Uretsky, adopted by me for the purpose of my analysis.

Regardless, the Norsworthy/Berndt argument relies on an improper application of the appropriate test. The correct procedure in fact supports the conclusion that there is no permanent input price differential.

(2) Drs Norsworthy and Berndt's attempt to justify the calculation of interstate-only TFP growth is based on an algebraic error which undermines their entire argument.

(3) Dr Nadiri offers only unsupported arguments in favor of AT&T's position that it is possible to calculate an economically meaningful interstate-only TFP growth rate. Indeed, he relies on a published paper of mine which contradicts one of his main arguments.

(4) ETI (on behalf of Ad Hoc), in an attempt to rebut my hypothesis testing results, supresses evidence which is inconsistent with its argument that there is a permanent input price differential. As a result, ETI's arguments are devoid of any legitimate economic meaning.

A. Response to Reply Statement of Dr. John R. Norsworthy and Dr. Ernst R. Berndt on Behalf of AT&T

2. In this section of my response I consider the Reply Statement of Drs. Norsworthy and Berndt which appears as Appendix B to Reply Comments of AT&T, March 1, 1996. I will concentrate on Drs. Norsworthy and Berndt's evaluation of my initial declaration regarding the input price differential, and on their attempts to justify the calculation of interstate service - specific TFP growth rates.

1. The Input Price Differential

3. Norsworthy and Berndt's use of times series procedures to criticize my analysis is actually an attack on the Bush-Uretsky methodology and the Bush-Uretsky regression equations. To the extent their analysis has any validity, which overall it does not, it discredits the Bush-Uretsky regression equations which appear in Appendix F of the FCC's Performance Review Order. It is logically impossible for Norsworthy and Berndt to continue to extol the virtues of the Bush and Uretsky equations while at the same time trying to discredit the equations in my analysis which use the same methodology. Indeed, Norsworthy and Berndt test the exact equations estimated by Bush and Uretsky, and find Bush and Uretsky's results to be spurious!

4. The essence of the Norsworthy-Berndt critique of the Bush-Uretsky methodology is their claim that the Bush-Uretsky equations (or the variations that I estimate) suffer from a basic problem sometimes encountered when using data drawn from a series of yearly observations. This basic problem is that even if some variables are truly unrelated to one another, a regression analysis will make it appear as if those variables were highly correlated with one another. Econometricians call such regression results spurious. When the regression results are spurious, any attempt to base conclusions on such regression equations are meaningless. Norsworthy and Berndt claim that the Bush-Uretsky regressions involving the input price differential are spurious regressions. To the extent their claim is correct, no implications concerning the input price differential can be drawn from such

regressions.

5. Fortunately for Bush and Uretsky (and myself), the Bush-Uretsky methodology survives the attack by Norsworthy and Berndt because Norsworthy-Berndt apply their time series procedures incorrectly in their tests.[1] The proper inference, when the Norsworthy-Berndt tests are carried out correctly, is that the regression results which use the LEC input price as the dependent variable may be spurious, and hence should be treated with suspicion. However, the regression results which use the LEC-U.S. input price *differential* as the dependent variable are not subject to the problems associated with spurious regressions, and remain valid despite Norsworthy and Berndt's claims to the contrary. The results based on these equations support the conclusion that the input price differential was a temporary phenomenon. The technical details of my critique of the Berndt-Norsworthy time series analysis is contained in Appendix A.

6. Norsworthy and Berndt at various times in their statement complain about the data I use and attempt to discredit my analysis on the basis that these data are suspect. They seem particularly concerned about the Moody bond yield (page 20). For some reason these complaints do not extend to the Bush-Uretsky analysis. This is exceedingly strange. All my data for the 1949-92 period were taken directly from the Bush-Uretsky data base as it appears in Appendix F. It is the identical data. AT&T was informed of this fact in a Bell Atlantic ex parte sent to AT&T on February 20,

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[1] Norsworthy and Berndt ignore a warning in the manual of the computer program they use (Time Series Processor (TSP) 4.3) that their cointegration test procedures are not valid procedures when a certain necessary prior mathematical test (called the unit root test) is not met. This failure occurs in one-half of their tests.

1996. The Moody bond yield data were introduced into the analysis by Bush and Uretsky - it is their series, not mine.[2]

2. Interstate Access Service Specific TFP

7. In my reply declaration in this proceeding, I demonstrated that it is not possible to calculate an economically meaningful separate TFP growth rate for interstate access services. In section C of their Statement, Norsworthy and Berndt derive an equation (equation (3), page 33) which they claim demonstrates how to separate TFP growth for interstate services from that for other services. In describing this equation they claim "this manipulation is algebraically valid."(page 33).

8. In fact, the manipulation is not algebraically valid. Equation (3) is based on their equation (2), which contains a basic algebraic error that completely invalidates equation (2), and hence also invalidates equation (3). The specific algebraic error is described in detail in Appendix B.

9. Equation (3) is the cornerstone of Norsworthy and Berndt's attempts to justify the existence of a TFP growth measure for interstate access services. Since this equation is shown to be invalid as a matter of basic algebra, their case for an economically meaningful service-specific TFP measure is without analytical support.

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[2] Bush and Uretsky give as their source of the Moody bond yield data The Economic Report of the President, 1994. Perhaps Norsworthy and Berndt's confusion arises from the fact that Bush and Uretsky mislabelled the series they used. They actually used the Moody Corporate Aaa bond yield rather than the public utility bond yield.

B. Response to the Statement of Dr. M. Ishaq Nadiri on Behalf of AT&T

10. In this section of my response I consider the Statement of Dr. M. Ishaq Nadiri which appears as Appendix C to Reply Comments of AT&T, March 1, 1996. In his statement Dr. Nadiri attempts to support AT&T's position that there exists an economically meaningful calculation of interstate access service-specific TFP growth rates. In fact, Dr. Nadiri presents no evidence which explains how an economically meaningful service-specific TFP growth measure could be calculated. His argument consists of three parts: (1) a repeat of the AT&T position that the FCC's cost allocation rules are economically meaningful, (2) a discussion of cost elasticities for toll and local services obtained from Canadian studies of Bell Canada data, and (3) a discussion of cost complementarity.[3]

11. In this response I will concentrate on the second and third pieces of evidence.[4] The fact that the FCC's cost allocation procedures cannot be used to obtain economically meaningful service-specific TFP growth measures has been thoroughly documented elsewhere in these proceedings.

12. Dr. Nadiri makes two points with respect to estimates of cost elasticities. He first claims that since the cost elasticity of toll service is smaller than the cost

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[3] Two outputs are said to be cost complements when an increase in the volume of one output decreases the marginal cost of the other output.

[4] The second piece of evidence is discussed below. The third piece of evidence is discussed in Appendix C. In that appendix I show that Nadiri's discussion of cost complementarity reaches an incorrect conclusion and therefore provides no support for the concept that a separate TFP growth rate can be calculated for an individual service.

elasticity of local service in two cited Canadian studies[5], this is evidence of increased efficiency of switched access service. This claim is incorrect. The relative levels of local and toll cost elasticities bear no relation to the question of relative efficiency growth rates. If this linkage were true, it would mean that smaller firms (i.e, those with smaller levels of inputs) would necessarily exhibit lower rates of TFP growth than larger firms that used larger levels of inputs. This of course makes no sense. Dr. Nadiri has mixed up absolute levels and growth rates.

13. The second claim is that because the cost elasticity of toll service was observed to decline faster than the cost elasticity of local service, this is evidence of relatively faster TFP growth in toll services. Nadiri offers no substantive support for the existence of a direct link between a decline in a service-specific cost elasticitity and a measure of service-specific TFP growth. In fact, no such direct link exists, nor could it, since service-specific TFP growth is not a meaningful concept.

14. The evidence quoted by Nadiri that the decline in the toll elasticity was faster than the decline in the local elasticity is based on an unpublished memo (Nadiri and Nanda (1995)). This memo was not included with Dr Nadiri's statement and therefore cannot be evaluated. Nadiri's evidence is contradicted by my paper (Denny, Fuss and Waverman (1981)) which was relied upon by Nadiri to support an earlier claim. In my paper the opposite result occurs: the local cost elasticity declines more

[5] The cost elasticity estimates in Bernstein (1989) are unreliable because of the peculiar specification of technical change which he employed that led to the following estimated empirical results for Bell Canada: During 1954-57 there was no technical change. In 1958 there was technical progress of 19%. During 1958-70 there was no technical change. In 1971 there was technical regress of 19%. During 1971-78 there was no technical change. Cost elasticity results which depend on this pattern of technical change cannot be taken seriously.

rapidly than the toll cost elasticity over the 1952-76 period[6].

15. I conclude that Dr. Nadiri has not presented any objective evidence which would point to the possibility of measuring TFP for interstate access services in an economically meaningful way. As I have stated earlier, it is impossible conceptually to find a method of measuring TFP for interstate services alone unless interstate services uses no inputs in common with intrastate services. This is clearly not the case. The input costs which are shared between interstate and intrastate services cannot be allocated in a an economically meaningful manner. Such an allocation is a precondition to economically meaningful estimation of TFP for interstate services alone, given the joint nature of the production process.

C. Response to the Reply Statement of Economics and Technology Inc. on Behalf of the Ad Hoc Telecommunications Users Committee

16. In this section, I consider the Reply Statement of Economics and Technology Inc. (ETI), which appears as an Attachment to Reply Comments of the Ad Hoc Telecommunications Users Committee, March 1, 1996. I will concentrate on ETI's evaluation of my initial declaration regarding the input price differential.

17. In my initial declaration, I demonstrated that the data used by Bush and Uretsky in Appendix F favoured the hypothesis that the LEC-US input price differential was a temporary phenomenon which ended around 1990. The hypothesis

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[6] Since Nadiri took the time to calculate average toll and local cost elasticities from the twenty-five yearly

adopted by Bush and Uretsky (and ETI), that an input price differential should be a permanent feature of a price caps plan for the LECs, was consistently rejected by the data in favour of the temporary change hypothesis. ETI contests these conclusions. It does not suggest that there is any error in my procedures. Rather, it claims that when the 1990 data point is dropped from the data set used in the analysis, the temporary change hypothesis is rejected by the non-nested hypothesis testing procedure. ETI then concludes that the permanent change hypothesis is the preferred hypothesis. ETI justifies the exclusion of this data based on the claim that the 1990 data point is an "outlier". ETI's conclusions cannot withstand scrutiny.

18. ETI's conclusions are based on a series of flawed procedures and a presentation of evidence which is blatantly selected to produce desired results.

19. In ETI's reply (tables A10 and A11) they argue that, when a part of my testing procedure[7] is applied to the equation with the LEC input price growth rate as the left hand side variable, using the Christensen 1 data set with the 1990 data point deleted, the temporary change hypothesis is rejected.

20. There are a number of errors in ETI's tables A10 and A11 which render ETI's entire analysis incorrect. First, ETI applies only one-half of the non-nested hypothesis testing procedure which must be applied in a correct application of the procedure. They incorrectly exclude the portions of the procedure which generate test

[7] In my initial declaration I used two testing procedures, the J- Test and the Cox Test. ETI restricts its reply arguments to an analysis of the Cox Test

results that reject their preferred conclusions.[8] Second, ETI applies the test to a regression equation which Norsworthy and Berndt, in their comments on behalf of AT&T, argue is spurious.[9] Third, ETI provides no argument as to why the 1990 data point should be considered an outlier, other than it is an inconvenient data point for someone who believes in the permanent change hypothesis. Econometricians have developed an objective analysis of outlier data, denoted the theory of influential outliers. When this theory is applied to the Christensen 1 data set used in tables A10 and A11 of the ETI submission, the 1990 data point is shown <u>not</u> to be an outlier.[10]

21. In its analyses of the regression equations submitted by NERA and Lincoln Telephone (tables A2-A9), ETI always presents two versions of the regression equation; one with the LEC input price growth rate as the left hand side variable, and a second version with the LEC-US input price *differential* growth rate as the left hand side variable. When we come to tables A10 and A11 (the Cox Tests), the second version of the regression equation is suddenly missing. This inconsistency can perhaps be explained by the fact that the test results applied to the second version of the regression equation for the 1949-92 period are inconvenient to ETI. Even with

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[8] They perform the part of the testing procedure where the temporary change hypothesis is the null hypothesis. Incorrectly, they do not perform the part of the testing procedure where the permanent change hypothesis is the null hypothesis. The Cox Non-Nested Hypothesis Test consists of <u>both</u> tests. For the regression equation and data ETI uses, this second part of the test results in a rejection of the permanent change hypothesis, a result which ETI perhaps finds inconvenient. For details please see Appendix D of this response.

[9] They ignore the equation which is not subject to the spurious regression criticism, the equation with the LEC-US input price differential as the left hand side variable.

[10] This result is based on a test which identifies outliers and is one of the testing procedures contained in the theory of influential outliers. The details of applying the theory of influential outliers to the 1990 data point is contained in Appendix D.

the inappropriate deletion of the 1990 data point, the temporary change hypothesis is accepted for this period, while the permanent change hypothesis is rejected[11]. The blatant selection of the evidence by ETI in an attempt to support its conclusions is totally unacceptable economic analysis.

22. In conclusion, ETI's attempt to discredit the validity of the temporary change hypothesis through the use of the non-nested hypothesis testing methodology fails on a number of grounds. They consistently and without explanation supress evidence that contradicts their arguments. As a result they offer no economically meaningful argument.

Reference

Judge, G.G, R.C. Hill, W.E. Griffiths, H. Lutkepohl, and T-C Lee (1988), Introduction to the Theory and Practice of Econometrics (second edition), John Wiley and Sons, New York.

[11] In this case, as in all other cases when the 1990 data point is deleted. the permanent change hypothesis is rejected. For a *complete* presentation of the test results, see Appendix D.